Difference of Longitude of Cambridge and Edinburgh.

Date. 1834.	N° of Stars observed.	Mean diff. of intervals.	Moon's hourly increase of R.A.	Diff. Long. in seconds of time.
		s	m s	s
Oct. 18	1	25,12	1 52,40	781,6
Nov. 14	3	24,93	1 51,24	784,2
Dec. 8	4	24,77	1 49,75	790,1
10	4	23,18	1 47,30	789,3
13	2	27,20	2 0,29	789,2
16	3	31,44	2 20,00	779,2
1835.				
Jan. 1	1	27,03	2 0,63	784,4
5	2	24,79	1 49,36	793,4
Feb. 6	3	24,39	1 47,56	794,3
8	1	30,78	2 14,86	793,1
10	3	33,19	2 25,05	787,1
12	2	33,10	2 22,48	805,8
March 6	4	27,91	2 2,77	792,8
8	4	30,87	2 16,35	786,5
10	1	32,11	2 23,50	775,7
11	2	32,45	2 22,93	787,5
12	1	31,78	2 20,16	784,1
Mean, allowing each result weight in the determination proportional to the number of observations on which it depends				788,7

	m s	
Long. Cambridge	— 0 23,54	*Cam. Phil. Trans.*
—— Edinburgh	+12 43,60	*Mem. Ast. Soc.*
Diff.	13 7,14	787,1
Diff. of determinations		1,6

III. Extract of a letter from Mr. Wrottesley to the Secretary.

" Towards the end of the year 1833 and the commencement of the present, I compared, on several occasions, the time at my observatory with that at the Royal Observatory, for the purpose of obtaining my longitude. The latter observatory is distant about a mile from mine, but is not visible from hence, the trees in the Park intercepting the view. The method I adopted, therefore, was to

compare my solar chronometer (Baird 2962), with my transit clock shewing sidereal time, a short time before one o'clock on each day. I then carried it to a spot in the Park, near the observatory, and observed the fall of the ball; noting the beat of the chronometer at which this took place. Having returned home immediately, I again compared my chronometer with my transit clock, usually at an interval of about 15^m; I thus obtained the correction for the chronometer's rate for the period between the falling of the ball and the second comparison, and, by converting this interval of mean solar time, thus corrected for rate, into sidereal, and subtracting it from that shewn by my clock at this second comparison, also corrected for rate, I obtained the time shewn by my clock when the ball fell. Correcting this time for the error of my clock, obtained (except on one occasion, the 18th December) by observations of the transits of stars on the morning or evening of the day, I had the true sidereal time at my observatory when the ball fell. These times, compared with the true sidereal times at which it was *observed* to fall at the Royal Observatory on the several days specified (with which Mr. Taylor was so kind as to supply me), gave the following differences or longitudes east from Greenwich, which are of course independent of any error in the time of the falling of the ball:

			s
1833.	Dec. 8	.....	(−2,31)
	... 10	.....	−3,11
	... 12	.....	(−2,24)
	... 13	.....	−3,49*
	... 14	.....	−3,10*
	... 17	.....	−2,67
	... 18	.....	−2,72*
	... 21	.....	−3,04
	... 27	.....	−3,01
	... 29	.....	−2,79
1834.	Jan. 1	.....	−3,28
	... 14	.....	−3,00
	... 16	.....	−2,90
	... 20	.....	−2,65
	Feb. 2	.....	−3,11
	... 5	.....	−2,49
	... 7	.....	−2,67*
	... 8	.....	−3,03

Mean of 16 comparisons, −2,94 E. Long. in time.

"I preferred this method to simple comparisons of the chronometer, from the consideration that it is easier to observe the time of the happening of an instantaneous phenomenon than to compare a chronometer accurately with a sidereal clock; and by substituting the ball for a comparison at Greenwich, I avoided errors arising from the latter. I considered also that the errors of the catalogue were insensible on an occasion of this kind, and that I could obtain

my clock error from the transits of stars with greater accuracy than the time of apparent noon from that of the sun. The comparisons with the sidereal clock registered were always the mean of several. On the four days marked with an asterisk the chronometer was not compared *before* the falling of the ball. The two values in parentheses, though not very discordant, have been omitted in taking the mean, because in both cases the beat of the chronometer at the time of ball-fall was determined in an unsatisfactory manner. I have computed my longitude also, from twenty-one observations, on the assumption that the ball falls exactly at 1^h mean solar time at Greenwich. The mean, rejecting none, gives $3^s{\cdot}05$ E. longitude, the extreme difference being $1^s{\cdot}64$. This result is not without interest, as affording a proof of the accuracy with which the ball is dropped.

" Upon the whole, therefore, considering the near agreement of the results, I may, without much risk of an error exceeding $0^s{,}20$, assume my longitude to be 3^s East."

Mr. Wrottesley also, at the same time, communicated several observations of the moon and moon-culminating stars; and also a comparison of the results obtained from the observations made on the same days at the Observatories of Greenwich and Cambridge, which shew the excellence of his instrument, and the great care bestowed on the observations.

IV. There was announced the receipt from Sir John Herschel of some printed " Instructions for making and registering meteorological observations," accompanied by a request that the council would give circulation to the following important recommendation of the Meteorological Committee of the South African Literary and Philosophical Institution, by whom those instructions have been framed: viz.

" With a view, however, to the better determining the laws of " the diurnal changes taking place in the atmosphere, and to the " obtaining a knowledge of the correspondence of its movements " and affections over great regions of the earth's surface, or even " over the whole globe, the Committee have resolved to recommend " that four days in each year should henceforth be especially set " apart by meteorologists in every part of the world, and devoted " to a most scrupulous and accurate registry of the state of the " barometer and thermometer; the direction and force of the wind; " the quantity, character, and distribution of clouds; and every " other particular of weather, throughout the whole twenty-four " hours of those days, and the adjoining six hours of the days " preceding and following.* The days they have been induced to

* " This is necessary, by reason of the want of coincidence of *the day* in different parts of the globe, arising from difference of longitude. In order to obtain a complete correspondence of observation for twenty-four successive hours over the whole globe, it must be taken into account that opposite longitudes differ twelve hours in their reckoning of time. By the arrangement in the text the whole of the *astronomical day* (from noon to noon) is embraced in each series, and no observer is required to watch two nights in succession."